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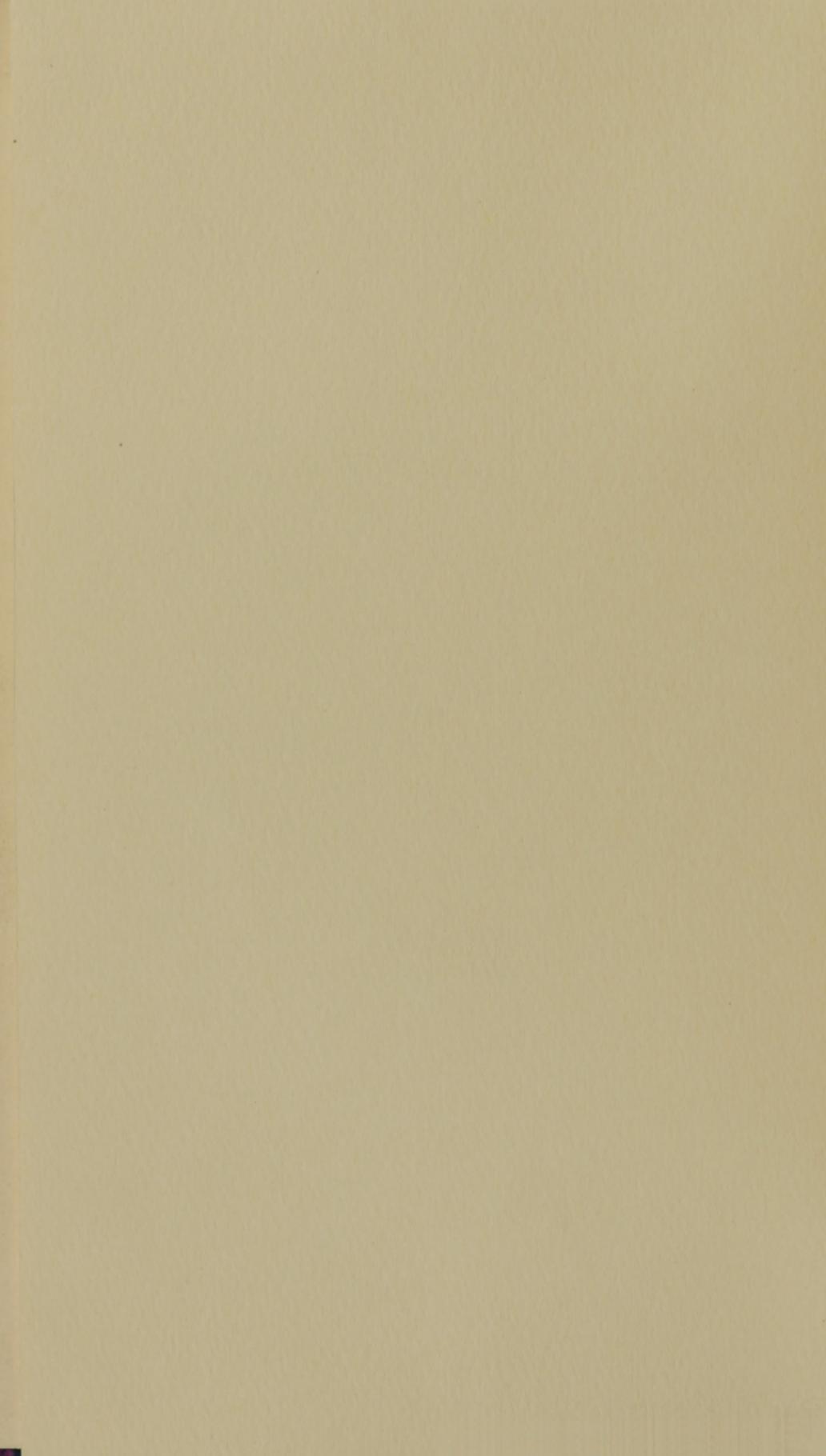
Section,

Essays Medical

ANNEX

No.

21363



AN EXPERIMENTAL ESSAY

ON THE

ABSORPTION OF MEDICINES.

BY JOHN C. GEDDY,

OF RICHMOND, VIRGINIA; HONORARY MEMBER OF THE PHILADELPHIA MEDICAL AND CHEMICAL SOCIETIES.

..... Pudor, et Justitiæ soror
Incorrupta Fides, nudaque Veritas.

HORACE.

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PHILADELPHIA:

PRINTED FOR THE AUTHOR, BY HUGH MAXWELL.

.....

1802.

TO DOCTOR JOHN CRINGAN,

OF VIRGINIA,

THIS ESSAY is inscribed, as a mark of respect,
from his sincere friend, and

Grateful pupil,

THE AUTHOR.

TO THE REV. JOHN BUCHANAN.

OF VIRGINIA.

SIR,

PERMIT me to dedicate this, my first production in the science of medicine, to you, not from mere custom, but from that esteem which I possess for your public and domestic conduct, which adorn you as a philanthropist.

With the most fervent wishes for your present and future happiness, I subscribe myself

Your friend,

THE AUTHOR.

TO

BENJAMIN SMITH BARTON, M. D.

PROFESSOR OF MATERIA MEDICA, NATURAL HISTORY,

AND BOTANY,

IN THE UNIVERSITY OF PENNSYLVANIA,

THIS ESSAY IS INSCRIBED,

AS A RESPECTFUL TRIBUTE

TO

PRE-EMINENT LITERARY

AND PROFESSIONAL MERIT,

AND

AS A GRATEFUL MEMORIAL OF ESTEEM

AND FRIENDSHIP,

BY THE AUTHOR.

TO

INTRODUCTION
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BY THE AUTHOR.

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It will not be expected that I shall refer to
all the different observations which have been

INTRODUCTION.

THE subject which I have thought proper to chuse for my inaugural thesis, is that of the absorption of medicines: a subject concerning which there appears to be a collision of opinions among physicians. It is not from that aspiring principle, the love of fame, that I have been induced to come thus forward before the public; but from the *duty* which devolves on me, at the present time. In the investigation of my task, I am persuaded that I shall not bestow on it that attention which it so eminently merits. The facts which I have collected, and the experiments which I have made, I now submit to the public. I sincerely regret that they are not such as I could wish they were.

It will not be expected that I shall refer to all the different observations which have been

made by ingenious men, possessed of lively imaginations, who have advanced opinions as the basis of particular theories. It is, probably, with some, the polar star of their speculations. In prosecuting this subject, I have no favourite theory to support, but aim at delivering facts, as they have occurred from experiments. Without experiments, it would be a mere hypothesis for any man to advance an opinion, however rational it might appear. For men of genius too often deviate from the path of judgment, with a wish to penetrate into the labyrinth of speculation.

It is from facts, and not from conjecture, that we can expect to arrive at that acmé which should be most sacred to man....truth. "As long as medical science is founded on imagined principles, assumed as data, its forms may vary daily; for it is the custom of the day, for one fashionable opinion to supplant another."

We deem it unnecessary to recite the advantages resulting from the establishment of the following experiments: they must be obvious to the most superficial observer in medicine.

ON THE

ABSORPTION OF MEDICINES.

THE experiments which will appear in the sequel, were made to ascertain whether absorption ever took place when certain substances were applied to the surface, which we will leave for the candid reader to determine. It is proper to mention, that more experiments were made than are here related. We have selected those which appeared to have been made with the most accuracy.

EXPERIMENT A.

Cataplasms of garlic were applied to my feet, which were placed out of a window; every caution was taken to prevent the air, in which my feet were placed, from penetrating

the lungs. To evade all doubt on the subject, I got my friend and fellow-graduate, Mr. Washington, to set by my side, for one hour and a half, at which period the garlic was taken off. My feet being washed, I walked out, returned in about one hour, at which time I examined the urine, but could not detect the smell of garlic...one hour after referred to the urine for a test, and found it impregnated with the smell. My friend Mr. Washington could not discover it in his urine or breath.

It being a fact well established, that if cattle feed upon garlic the milk becomes strongly impregnated with the smell and taste of this vegetable, I was desirous to know, whether this smell depended on absorption or on transudation. I, accordingly, instituted the following experiment.

EXPERIMENT B.

Having obtained the liberty of a female who gave milk, to apply garlic to the surface, I proceeded in the following manner: I placed her near a window with a long tube, one end of which she took in her mouth, whilst the other was placed out of a small hole in the window: her nose being stopped she breathed

through the tube. I, then, applied a large quantity of garlic to her feet, which had been previously immersed in warm water. She continued in this situation for two hours and a half, when the garlic was taken off. She then retired to another room. I examined the milk and found it slightly impregnated with the smell of garlic. The breath also smelt of it. I was desirous to examine her urine, but the delicacy of the occasion prevented a farther inquiry. After making these two experiments, I found the spirit of inquiry extending still farther; when it was intimated by professor Barton, that camphire had been detected in the breath and perspirable matter of persons who had taken this active medicine into their stomach. To satisfy myself on this head, I made use of the camphire, in the following manner.

EXPERIMENT C.

Being situated, as related in experiment B, with the tube, &c. my friends Messrs. Price and Washington applied a quantity of camphire, finely powdered, and mixed with spirit, to my feet, which had been immersed in warm water, prior to the application. The camphire was continued for one hour and a

half, when it was taken off, and my feet washed. I continued to make use of the tube until I left the room, when my breath and urine were examined by the above mentioned gentlemen. The former (Mr. Price) was of opinion, that he could smell the camphire in the breath; the latter (Mr. Washington) did not think he could smell it: both were of opinion they could not smell it in the urine. I think it proper to observe, that neither of these gentlemen had much confidence in the doctrine of the absorption of medicines.

EXPERIMENT D.

I took about five drops of the spirit of turpentine, diluted, to ascertain whether there would be the smell of violets in the urine. This smell, it is well known, is the general characteristic of the presence of turpentine in the urine. In about one hour, I found the urine possessed of a strong violet smell.

EXPERIMENT E.

Five days after making the last experiment, (that in which I took the urine for the most conclusive test)...I had a quantity of the spirit of turpentine poured into a large vessel

in a close room. I walked backwards and forwards, for one hour, at which time I had my urine examined by my friends. They could not detect the smell of violets. I was induced to make this experiment, inasmuch as some writers have thought the lungs the only apparatus necessary for the office of absorption.

The weather being much condensed when the last experiment was made, I repeated it, on a very fine day, with the same result. On the eighth of March, the following experiment was made, with the necessary precaution.

EXPERIMENT F.

Having taken the extremity of a tube in my mouth, disposed as described in the preceding experiments, and nostrils properly stopped, I requested my assistant to pour a large portion of the spirit of turpentine on my feet. I remained in this situation for one hour, when the usual test was referred to, by the gentlemen formerly mentioned. Not the least smell of violets, agreeably to their assertion, was perceivable.

These experiments, made with the spirit of turpentine, appeared very conclusive. Inflammation not taking place in the part which was immersed in the turpentine, as related by

some writers....produced a suspicion in the minds of my friends, that the turpentine was not so pure as it might be. Wishing to avail myself of an opportunity to remove every doubt on the subject, I obtained a fresh supply of the spirit of turpentine, with which I made the following experiments.

EXPERIMENT G.

I exposed myself, for one hour, to the emanations proceeding from the spirit of turpentine. I, then, had my urine examined. At this time, it was void of the smell....after walking for one hour, I found the urine, which I now evacuated, slightly impregnated with the violet odour.

As the last experiment obviously militated against the former experiment, marked E, I was rather induced to believe a contrary result might take place with the others. To ascertain the point, the following experiments were instituted.

EXPERIMENT H.

After arranging all things, as formerly mentioned, with the tube, &c. a servant rubbed my hands and feet, for one hour, with the spirit of turpentine. I had them well washed

and left the room, there was a great degree of inflammation produced and a disagreeable pricking sensation, which continued for upwards of twelve hours. The urine was not examined until one hour after I retired from the room, where the experiment was made; at this time, it was strongly impregnated with the smell of violets.

Having some doubt existing in my mind, that the cuticle was destroyed in making the last experiment, and that friction might have assisted the powers of absorption, several days after all inflammation had disappeared, and the urine did not any longer possess the smell of violets, I proceeded in the following manner.

EXPERIMENT I.

Precautions being taken, that the atmosphere where the spirit of turpentine was placed, should not be inhaled, I placed my feet in a vessel filled with the turpentine: it was my intention, for them to have continued for two hours; but finding the inflammation very extensive and much pain produced, I had the turpentine removed in half an hour, my feet being washed. We now had recourse to the usual test (the urine), but could not discover

the smell of violets. One hour after, I discharged the contents of the bladder: my water was strongly impregnated with the smell of violets, which continued from this time, which was one o'clock, until twelve at night. This experiment was repeated several times, with the same result.

Mr. Sherrvin, and some other writers having asserted, that the external application of tartar emetic would produce nausea and puking, without mentioning whether there was any precaution taken to prevent the emanations from entering the lungs, which might be said to produce the effect, I made the following

EXPERIMENT K.

Breathing through a tube, I had about two drams of tartar emetic rubbed on my hands. In about two hours and a half, I had a slight nausea: the friction was discontinued at the above mentioned period; the nausea continued for some time after I left the room.

EXPERIMENT L.

After preparing a couple of bladders, I had the insides of my thighs covered with the

unguentum bydrargyri fortius, which were covered with the bladders, the edges of which were kept in close contact with the skin by adhesive plaster. I continued them for forty-eight hours, at the end of which time I experienced that peculiar copper-taste which is generally produced from the use of mercury: my mouth was not sore, nor could I discover that the glands discharged a larger quantity of saliva than usual; the tongue was much furred, which was not the case before the application of the mercury. The bladders were made use of to prevent the emanations of the mercury being inhaled. The late Dr. Cooper has related a case of a female who was salivated, producing the same affection in a child sucking her.

My friend and fellow-graduate, Mr. M'Donald, having heard the result of my last experiment, was so kind as to favour me with the following letter;

“ DEAR SIR,

“ Knowing you are now engaged in writing a dissertation on the absorption of medicines into the circulation, I take the liberty of transmitting to you a fact, in corroboration of an experiment made by you, a few days past.

“ In the year 1798, there was a gentleman in Winchester, Virginia, who was affected with syphilis, and knowing the cure depended chiefly on the application of mercury, he was hardy enough to undertake the management of the disease himself. Being aware that the medicine would discolour gold, he was particularly careful of his watch; so much so, that even after taking a pill, he used the precaution of washing his fingers, not having an idea that the medicine taken by the mouth, could make its way through the system and attack his watch in his fob, which was the case: for as soon as his gums began to get sore, his watch began to lose its colour, and in a short time was rendered quite white.

“ This is the history of the case, as I received it from the gentleman. I think it may be more especially depended on, as the gentleman was not of the medical profession, of course was biassed by no pre-conceived theory or opinion, but simply related the case to me as a curious fact. I inquired particularly of him, whether he had used any of the medicine by friction: he declared he had not, but, on the contrary, always used the precaution before stated. Unless there was some falacy in the experiment, which I am not aware of, and which escaped the attention of the gen-

tleman, I think it amounts almost to a demonstration, that medicines may be taken into the circulation.

“ I am yours, &c.

“ T. M'DONALD.”

From these Experiments, it must appear, that certain substances may be absorbed, when they are applied to the surface of the body. We are also induced to believe that the lungs possess this power; but do not think them the only organs in performing this function. A number of cases might be related of persons being salivated from visiting patients labouring under ptyalism. A very striking case of this kind occurred in the Pennsylvania Hospital, during the last winter, of a gentleman who often attended the venereal ward, having a copious flow of saliva. In establishing the absorption by the lungs it will not militate against absorption from the surface.

We will now proceed to take notice of some few objections made to absorption. We will first attempt to prove, that certain substances applied externally produce the same effects as if taken internally. Secondly, that certain active substances may exist in the blood-vessels, without producing death; thirdly, that the fluids are impregnated with those substances taken internally.

Dr. Monro applied camphire to a frog, and found it absorbed. Dr. Barton mentions a patient under his care in typhus, to whom he gave the medicine. On raising the bed-clothes, he discovered the smell. A copious perspiration came on, which was found to smell strongly of camphire. This active article externally applied has produced palsy: a case from this cause came under the notice of the Doctor.

Professor Bergius has informed us, that when opium is applied externally, it produces sleep. Dr. Whytt always found when the state of the stomach was such that it would not admit of the exhibition of opium internally, that he could produce the same good effects when externally applied.

Lead applied externally has, in many cases, produced palsy similar to that in Colica pictonum; a number of such cases we have on record*. Dropsy has been cured by the external application of squills, which are known to be powerfully diuretic. Strangury is very often produced by the application of blisters, which we would explain upon the principle of absorption. If this effect depended upon sympathy, we should experience the same effects from any of the siliquosæ; “neither mus-

* Baker.

tard, the actual or potential cautery, nor any other vesicating stimulus, but cantharides, excite this complaint. A blister, laid upon the head immediately after shaving, is almost always succeeded by strangury: whereas no such effect takes place, if the application be delayed twenty-four hours.”*

Tobacco applied externally produces nausea and puking, which are the effects of this article when taken internally.†

Litharge carried under the arm-pits caused dyspnœa, fainting, nausea, vomiting, &c. Ceruse applied to a part that had been chafed, produced similar effects ‡ The application of Goulard’s poultice to the skin, continued for some time, produced a violent pain in the bowels, which did not cease until the removal of the poultice. Instances have occurred of convulsions being produced in children by ceruse sprinkled on excoriated parts. It is not improbable that litharge, the common basis of plasters, when employed in dressing issues, produces some of the common effects of the preparations of lead taken internally. || Unfashionable as it is, in the commencement of the nineteenth century, to refer to the records of antiquity for authority, we must avail

* Percival. † Dr. Barton. ‡ Haller. || Medical Trans.

ourselves of the opportunity to support modern inquiries. The ancient writers may be considered as the oracles of truth: if they have erred, it was from the want of judgment. It is a common practice with physicians to apply the bark externally for the cure of intermittents. It is said not to act by absorption, but by sympathy. This term involves as many, and we may say, more difficulties than absorption. From the present prevailing opinions, certain circumstances are referred to it which may be explained upon more philosophical principles.

If it depends upon this vague term called sympathy to produce those good effects which we observe daily, why will not other substances answer as well, for instance the muriate of ammoniac? The oxygenous and carbonic gases are very readily absorbed, whilst the nitrous, hydrogenous, and nitrogenous gases tardily gain admittance into the absorbing vessels. Two ounces and a half of carbonic gas were absorbed in five hours; eight ounces of oxygenous gas were absorbed from immersing the hand eight hours in the gas*. From a series of experiments made by this writer on the functions of the skin, we

* Abernethy on the Functions of the Skin.

are obliged to conclude, that its surface is possessed of the power of absorbing. If the perspired carbonic gas, be confined by our garments, it seems likely that it will be taken up again by the absorbents. Whether the body does usually imbibe water from the atmosphere, adequate to the loss sustained by aqueous perspiration, is uncertain. I am inclined, however, to suppose that the absorption of air from the skin, is greater than the secretion. The great quantity of water which the skin can occasionally imbibe from the atmosphere, is evinced by people in a dropsy.

The celebrated De Haen, finding that his dropsical patients filled equally fast, whether they were permitted to take liquids or not, did not hesitate to assert, that they must absorb from the atmosphere. That the surface of the skin absorbs other fluids, which may come in contact with it, I have not the least doubt. "A patient of mine, with a stricture in the œsophagus, received nothing either solid or fluid, into the stomach, for two months; he was exceeding thirsty, and complained of making no water. I ordered him the warm-bath, for an hour, evening and morning, for a month; his thirst vanished, and he made water in the same manner as when he used to drink by

the mouth, and when the fluid descended readily into the stomach.”*

Different substances may be injected into the blood-vessels of animals, without producing death. This will appear from the following facts. Dr. Roebuck injected a quantity of bile into the blood-vessels of a dog, without any ill consequences resulting from the same. From an experiment made by this ingenious author, we may assert, that it is the absorption of bile which produces that yellow colour which is known to exist in persons affected with jaundice; not merely the colouring matter, as has been thought by some writers. He also went so far as to examine the blood and urine of a person labouring under the disease of jaundice, and found them to strike a green colour with the muriatic acid. This was proven to be the best test by his making several experiments with the urine and blood of persons affected with other diseases and those in health. Dr. Sybert injected a quantity of ammoniac, with equal impunity, into the blood-vessels of a dog: it could not be detected by any test. Although a substance cannot be detected, we are not positively to con-

* Cruikshank on the Absorbents.

clude that it does not exist. Some more happy æra, I hope, is approaching when we may penetrate into the recesses of nature, although enveloped as it may appear at this time.

We have it stated on record, that matter which produce contagious diseases can circulate in the sanguiferous system, for a length of time, without injury. Why may not medicines, which are not more deleterious, enter the system, with equal impunity? It is thought, by physiologists, that the chyle does not assimilate immediately with the blood after entering the left subclavian vein. We believe it may, for a considerable length of time, float on the blood in its pure state. The following facts may be advanced in support of the opinion. "A maid, after eating a good breakfast about seven in the morning, was let blood about eleven the same day, in her foot. The first blood was received in a porringer, and within a little while it turned very white. The last blood was received in a saucer, which turned white immediately like the white of a custard. Within five or six hours after, I chanced to see both, and that in the porringer was half blood and half chyle, swimming upon it like a serum as white as milk; and that in the saucer all chyle, without the least appearance of a drop of blood: And when we heated them

separately over a gentle fire, both hardened as the white of an egg, or just as the serum of the blood doth with heating, but far more white. This maid was then in good health, and only let blood, because she never had her courses, yet of a very florid clear complexion.”*

A similar case came under my notice, in the course of the last winter. A gentleman who had a severe affection of the head after eating a large supper, two hours after, let about twenty ounces of blood, at one bleeding. The blood was kept until morning, when it was examined by several medical gentlemen in Philadelphia—it had separated: there was found floating upon the surface, a white cream-like fluid, which was proven to be chyle, from its coagulating by heat, even sooner than the serum did. There appeared to be about two ounces of chyle in the quantity drawn.† We are informed, by Haller, that if either medicines or poisons are injected into the blood-vessels, they there produce their several determinate effects; such as nausea and puking from the stomach, purging from the intestines and a comatose state of the system.

* Edinburgh Medical Essays, vol. v.

† Barton's Lectures on Materia Medica.

It is a well authenticated fact, that the lues venerea is communicated from the mother to the fœtus *in utero*. Several cases might be mentioned of the communication of the small pox and measles, in the same manner. The author had an opportunity of seeing a female labouring under the measles; she was delivered of a child infected with the same disease.

Mr. Turnbull relates the case of a lady, who was inoculated in the seventh month of her pregnancy; nine days after the eruption she received a fall, and in a few days after that was delivered of a dead child, which was covered with variolous pustules, in a state of suppuration. The matter was proved to be variolous from its communicating the disease to several persons, who were inoculated with it.* Certain substances, when taken internally, manifest their presence in the fluids either from their taste, their odour, their colour, or effects. If the spirit of turpentine be taken internally, the urine is observed to have a strong smell of violets. Asparagus, in like manner, communicates a strong and disagreeable smell to the urine.

Dr. Barton takes notice, in his lectures on materia medica, of a nurse who had

* Memoirs of the London Med. So. vol. iv. page 364.

taken opium, which affected the child in the same manner as if it had been exhibited to itself. A nurse having taken a purgative medicine, gave suck to a child: a hypercatharsis was induced on the child, although the nurse felt no ill effects from the medicine. It is a well-known fact, that cathartics are often given to the nurse with the effect of evacuating the intestines of the child. How often do we observe that a particular regimen of a nurse will produce constipation and cholic in the child, and when changed the child is relieved from all those distressing symptoms?

We often observe the urine of persons taking radix rhæi highly coloured; this is accounted for, by some writers, on the supposition, that all substances are possessed of a nutritious quality, and it is this part only which is carried into the circulation. This is certainly, arguing from false facts, if I may be allowed the term: for, surely, no person will say the rust of iron or gum-kino are *nutrientia*. From the preceding experiments, we are rather induced to believe, that the spirit of turpentine is actually absorbed, and we cannot think any but men of delusive imaginations would think of placing this acrid article under the head of *nutrientia*.

The deer are known to feed on the *Kalmia latifolia*, or broad-leaved American laurel, with impunity; but when the animal is slain, and immediately prepared for the table, we find those persons who eat of its flesh are affected with a violent purging. Sheep that feed on scurvy grass, taste very strong of that vegetable.* We are told a species of fish which are caught in the West-Indies, called barracuta (*perca major*, of Brown) are poisonous if taken as food, unless they are gutted and salted, as soon as they are caught: they are said then not to produce those deleterious effects. I have been informed, by several gentlemen who have seen a number of those fish caught, and all those precautions taken which are recorded in the memoirs of the medical society of London, for preparing them for diet, that those persons who eat of them were diseased in the same manner as when the above precautions were not taken.

We believe, however, that these fish are not so apt to produce their ill effects if the intestines be taken out immediately, as when they are suffered to continue for any length of time in the body; which may be accounted for here in the same manner as with respect

* Barton's Lectures on Materia Medica.

to other animals, by supposing that the intestines increase the process of putrefaction, from the acrid *fæces* which they contain. This is very ingeniously accounted for by a late elegant writer, in the following manner. He is of an opinion, that as soon as death takes place, the different reservoirs begin to lose the power of containing their respective contents; and that, hence, transudation is the consequence.* I am sorry to be obliged to oppose such an authority: but I am of a contrary opinion; for we observe that the hearts of animals continue to vibrate for a considerable length of time after the body appears to be deprived of all its vital functions. May we not, then, with equal justice, conclude, that the different reservoirs possess this contractile power? It is not compatible with the laws of the animal economy, to suppose that different fluids can pervade the system until the muscular fibre is destroyed by putrefaction or impervious by vessels. If fluids get *out* of vessels by transudation, they may get *into* them in the same manner; for instance, we should be able to detect the urine in the blood-vessels, and the chyle in the ventricles of the brain, neither of which discoveries has ever been made.

* Dr. Hodge.

We often observe the abdominal muscles, considerably tinged of a green colour, after death has taken place ; but we cannot pretend to say, that the colour, in this instance, depends on the transudation of bile. On opening the body, we find the gall-badder still retaining its contents, and the adjacent parts not in the least tinged with its colour. It would be preposterous to say that the urine transudes from the bladder into the thorax, or the bile from the gall-bladder to the extremities. Equally just the above assertions would be, with those, that the fluids of an animal transude throughout the body, to produce those deleterious effects experienced from the use of the American pheasant,* which has fed on the berries of the laurel, already mentioned, or the deer that has fed on the leaves, &c. of the same vegetable.

I shall not pretend to arrogate to myself, the presumption to say, these observations are sufficient to establish the truth. The difficulties are innumerable which attend an experimenter. Should it be the happy fate of some more indefatigable genius to explain this part of physiology, I shall feel myself contented either in being supported or confuted.

Stulti sunt, qui corrigi nolunt.

* *Tetrao Cupido.*

Med. Hist.

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